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CONTENTS

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MEDICINE AND GROWTH[1]

DOUBTLESS friends have congratulated you on the fact that you were "through." In one sense—a strictly academic sense—that is true, else you would not be here, subject to this ordeal. But perhaps there is another way of looking at the situation. The Greek philosopher, Pyrrho, contended that against every statement the contradictory may be advanced with equal reason, and following this estimable skeptic, I feel justified in the assertion that, as a matter of fact, you are not "through," but rather are just commencing doctor of medicine, if one may give the word commence its older meaning.

You have qualified for a degree—a degree which entitles you to membership in a learned profession—that, like the church and the law, has the distinguishing responsibility of dealing with matters of life and death.

A profession makes heavier claims on its representatives than does a trade or an art, for in the nature of the case it demands continued progress, and it is part of the unwritten law that those who enjoy the prestige which such a position brings, should leave their profession better than they found it.

To do this implies progress—progress by growth, and it is the idea of growth that I wish to use as a guiding thread for the conduct of this talk. It is my purpose then to say a word concerning growth as it affects that very important person, the patient; then to speak of growth as it touches the body of medical knowledge; and finally to consider growth as it affects the physician in his riper years.

To follow an old time form let me announce

[1] Address to the graduates of the Medical Department of New York University. Delivered at the special commencement exercises, held at University Heights, New York, on Saturday, March 1, 1919.

my first thesis, namely: that a patient is always changing, growing.

Johannis Bernoulli, a member of the most remarkable family of mathematicians of which we have a record, published in 1699 a thesis in which he maintained the continual change of substance of the body.

His argument drew the theological lightning of his day, and he forebore to push his studies further, but his ideas were passed along, and I know that in my youth no self-respecting popular physiology failed to repeat the statement that the human body underwent a complete change of substance once in seven years.

We look at matters somewhat differently today but it is not without interest to record that this idea of change started under such eminent patronage. The modifications due to growth are another matter, yet the idea of growth, despite the universality of the phenomenon, has been only gradually assimilated and put to use.

In earlier times growth was but little considered. We need not go back very far in medical history to find that the typical patient was the person already grown. The patient was thus standardized.

The young were dealt with by midwives and grandmothers, and the aged took care of one another.

Speaking in the broadest way the physician's business was to care for that mythical person, the average man, to whom the recorded facts of anatomy and physiology all applied; for the phases of growth were not then regarded in these disciplines, and medicine shared with art and education a curious blindness to developmental changes. Great advances have occurred. We now have those clinicians who give special care to children, to adolescents or to the aged.

The relations of age to the incidence of disease, as in the children's diseases, in typhoid or in cancer, have directed attention to progressive alterations within the individual, a series of changes which are quite aside from the marks of maturity or the signs of old age.

Thus men of a given race pass through a series of well recognized phases and, as in a set of dissolving views, infancy merges into childhood and childhood is transformed to youth, and so within the span of life we have revealed the seven ages of man, so quaintly sketched by Shakespeare.

Familiar as these phases are it has taken no small labor to bring them into the field of practise and to have them recognized as of clinical importance. There is the same difficulty here that appears in carrying over to our laboratory work the ideas of variability and of graded relationship which were developed by Darwin and those who followed in his steps.

We know that individuals differ in their form and anatomy, but we wish they didn't; it would be so much easier if they were all just alike.

We know, too, that what is true of structure is also true with regard to the functions of the body. Here the facts are harder to appraise, and there is a still stronger tendency to dodge them. But this avails us nothing. The facts will find us out—and moreover they are unpleasantly immortal.

The idea which I wish to drive home is this: During the span of life the body shows changes more or less like those shown by a battered ship or neglected automobile, but behind these lies a set of changes which no dead structure or machine exhibits, a progressive chemical alteration of the body linked with age, probably affecting all its parts, and constituting the series of modifications characteristic for the individuals of any species, as these pass from birth to senile death.

The mechanism which prepares our food; that which distributes the food-bearing blood; the nervous system which controls our behavior; the muscles which do the work, and the internal secretions from the ductless glands and other sources which serve to tune or tone our organs, all these undergo with age changes not only in themselves but in their relations to one another.

On the balance of these component parts depends that somewhat subtle character called

temperament, which though elusive, has a real existence and an importance hard to overestimate. Temperament is the expression of these relations and one of the nice problems the clinician has to face.

Under certain circumstances it makes a difference whether one has light hair or dark, not because these characters are themselves important, but because they are indicative of subtle dissimilarities in the chemistry of different individuals, dissimilarities which are of far-reaching importance for the individual as a whole.

Recently it has become possible to do our laboratory work with animals the ages of which are known. Working thus we find at every turn differences, distinct and definite, dependent on the age, differences which should be studied, for without shadow of doubt they will be found in man when search for them is made.

I make no question that much of what I have just said to you has a familiar sound, but the time is coming, I feel sure, when the significance of age will be appreciated in many fields where now it is but little noted, as for example in the blood, and I have spoken thus to specially direct your thought to these matters.

Thus far the individual who is growing normally and who represents the usual case has been considered. In passing, however, it may be worth while to turn for a moment to the individual subjected to starvation. The terrible years through which the world has just passed have brought starvation vividly before us. We know that in starvation growth is modified and may apparently be stopped.

As in so many other instances our knowledge of the changes thus induced is still fragmentary and incomplete. In the first place we must distinguish between the starvation which follows when the quantity of an adequate diet is made unduly small, and the case in which the diet is unbalanced and defective in itself, and therefore only slightly modified by quantitative variation. It is the former case to which I would draw attention here.

If we may trust the tests with animals, two systems tend strongly to resist mere quantitative underfeeding—the skeleton and the nervous system. Growth in them is greatly retarded to be sure by underfeeding, but they may still grow, while the body as a whole is held at a constant weight or is even losing.

The practical question before us however is not so much the immediate effects of starvation, as the response which such an animal will make when it is brought back to a full and normal food supply.

The nervous system is best known to me and I think we may say with regard to this system that a return to the normal diet is followed by nearly, if not quite, complete recovery. This is a cheering and hopeful result and yet, as always, a word of caution is in place. Starvation, as followed in the laboratory, can be studied free from the complicating conditions of the exhausting systemic diseases, so often associated with starvation in human communities, and what is true for the simple conditions of the laboratory may not be true for those which are more complex.

Nevertheless in these days, when underfeeding is much in evidence, it is of interest to note that one form of it at least does not cause permanent damage to the great master system of the body.

The life histories of many students and productive scholars support this conclusion, for biographies show only too frequently, periods of starvation in the lives of those who, then and later, were distinguished for intellectual activity.

Thus far I have been speaking of growth as it modifies the patient, when that long-suffering person is looked at as a biological problem.

Now let me pass to the second topic and ask you to consider the growth of medical knowledge.

The mass of knowledge in any subject may be likened to a sphere which is rolled on from generation to generation, always growing by additions on the surface.

All of us, as scholars or investigators, are entrusted with its preservation and its in-

crease, but like the sacred beetles that also have their sphere, we often roll our load with clumsy slowness and humorous mishaps. Nevertheless this sphere contains our intellectual pabulum and is worth close scrutiny.

In the first place it is to be observed that like the moon the apparent size of this sphere is highly variable. When we first view it in the early student years it appears to have moderate dimensions. Later it seems enormous, but as the years go on it shrinks once more, and I venture to think that in this last phase our impressions correspond more nearly to reality.

An analysis of this experience may be worth while. The mass before us is typified by all that has been written plus the traditional wisdom which is handed down from teacher to pupil. When the written records are examined it becomes evident that the greater portion of them are formed by an enormous accumulation of evidence and arguments for a relatively small number of important conclusions, and also for a multitude of hypotheses which have perished by the way.

Did you ever go into a well-stocked library in which the books dealing with a given subject were arranged in their historical sequence, and then ask yourself what could be said of these—what was their larger meaning? It is worth doing. One can, of course, dismiss the greater number as out of date, a few only have the power to remain alive. Yet all these books, or nearly all, passed through a period when they were consulted and esteemed.

It is plain that most of our medical literature, including that which represents the fundamental sciences, is concerned with the presentation of evidence and arguments for some point of view. In the end the conclusions can be stated in a few words. When these conclusions are established and made certain, much of the literature developed by the way becomes of historic interest only, to be treasured and preserved of course, but removed from the field of central vision.

Thus when malaria was shown to be due to pathogenic organisms—insectborne—the antecedent literature concerned with other theories of its etiology ceased to be intructive.

One result of recognizing such a change is to make the sphere of knowledge seem less ponderous, yet it is never a small matter, and there is always with us the question how we can best handle this load of learning. In many cases it is necessary to carry only a skeleton outline of the existing knowledge, yet one must be ever ready to follow a subject into all of its details, when the occasion demands.

All this takes time and time presses ceaselessly. Always we have with us the stubborn fact that three score and ten years make a full life, and that although the day may be shortened by legislative action, no hours can be added.

Joseph Leidy, the distinguished naturalist, once said that he could carry some 20,000 names in mind, but if new ones were to be acquired, some of the old ones must be forgotten.

This is a somewhat cryptic saying and invites psychological analysis, but it also serves to direct attention to the limitations which beset even those exceptionally endowed. Apparently we only carry those facts which from time to time can be recalled. Neglect them and they get lost: like foraging pigs they must be called in now and then or they will forget the way home.

This sphere of learning which we have in view is composed of facts that date from many centuries. Some are surely very ancient, and strictly new ideas are hard to come by. Our classical friends are fond of pointing out that many ideas which we parade today were known to the Greek philosophers 2,000 years ago. Of course if persons make a business of thinking, as did this group among the ancients, they are bound to reach a number of more or less logical conclusions, though some of them may be quite contradictory. It was not until such rival conclusions could be put to the experimental test that it was possible to sift the true from the false, and therefore our biological science deals with no small number of ancient ideas

and the contribution of the modern workers is the selection of those which can survive the trial. As the expression goes: we test the hypothesis. Ideas which can survive the blows and buffets of time have a certain prestige and dignity which is to be reckoned to their credit, while the number born to perish, those that appear but once, are as the eggs of a fish. Moreover, it is most important for us in weighing the worth of ideas to know something of their history—I might almost say their ancestry—and not to confuse the unbaked results of the hour with those that have an ancient lineage. If we realize then the persistent character of all first-class problems, it ceases to be a wonder that when the results of an investigation are followed back into the literature, some of them may be found foreshadowed there or even definitely formulated, sometimes on good grounds, sometimes on bad.

Of course there are critical points in the advancement of knowledge which, when passed, make possible conclusions that are plainly novel and could not have been reached before. The aspect of medicine changed after Harvey discovered the circulation of the blood. The heat of the body appeared in a new light after Lavoisier developed the theory of oxidation. Galvani's observations on the nerves and muscles of frogs gave a new idea of the nervous impulse, and Johannes Müller's doctrine of the specific energies of nerves revolutionized our notions of the sense organs. Infectious disease suddenly became intelligible in the light of the work of Pasteur, and the doctrine of internal secretions and the chemical messengers, taking its departure from the observations of Brown-Sequard, shed a world of light on the control of body functions and relieved the nervous system from responsibilities which were proving too heavy for it.

If then we come back to our sphere of knowledge and endeavor to see in what manner it is compounded, we find in it ideas which repeat themselves at every revolution. We find great masses of information which, because they have served their purpose in establishing points of view, now have mainly an historical interest, and overlying all, most conspicuous and best known is the newer knowledge, the kind you have just labored to acquire, composed of these elements in part but in larger part consisting of detailed evidence, valuable for the newer points of view.

It requires some skill to manipulate this mass of information without being smothered by the dust of it, but handbooks, summaries, digests, reviews and journals deal with it in such a way that one can get their bearings with a comparatively small expenditure of time.

There still remains the question how this information by which we live should be regarded. There have been communities and times when medical learning was handled almost as a trade secret, indeed the Hippocratic oath suggests that this idea was an ancient one. It was as though the possessor had acquired a fixed and rigid formula for making a peculiarly good article useful to the public, but the production of which should be protected. This attitude has been abandoned happily, save in the most backward communities and among the least intelligent practitioners. The modern and the progressive view is quite different. It is in harmony with the response of John Hunter the great comparative anatomist, when some one quoted to him a statement which he had made a year before. "Sir," he replied, "I am not to be held by my statements of a year ago." He had progressed in the interval and he had accordingly changed his opinion. The knowledge which has been presented to you, and to which you have added by your own industry, is to be sure the best available at the moment, but that is the most that can be said of medicine and the most that can be said of any science. If we believed otherwise, if for a moment we thought of it as fixed and final, those of us at least who work in laboratories would promptly go into the chrysalis stage and somnolently wait for immortality. That however is not done. To-day the best use possible is made of such information as we have been able to gather, but with the confident expectation that to-morrow will bring new knowledge. Look at the extension of our

notion of the ether familiar to most of you in the Röntgen rays, or the sudden widening of the chemical horizon by the discovery of radium and the analysis of the atom into its constituent electrons. These new ideas make the older men to think, a painful process, and, because painful, avoided if possible.

Now, even in medicine, there are difficulties of this sort which create a somewhat trying situation.

I have been appalled by the apathy of some of my medical friends towards the experimental work which goes on in the laboratory. We need encouragement and protection in the laboratories, especially for such work as involves the study of the living animal. Vivisection this is called by those who oppose the study of the living animal—but as there is no essential difference between this work and either surgery or medicine—by the same token both surgery and medicine are vivisection. So we may compromise, and speak of this operative work as animal surgery or medicine. Many studies require to be made on the living animal but here in this community, and those communities in which my lot has been cast, such study is often strenuously opposed by some who will not see its value. I had supposed that my clinical friends, representing as they do the most influential group of professional men at the present day, would rise in a body and say *this work is necessary for our progress and the advancement of our profession*—but they did not. I tried to find out why.

Various reasons appeared—some of which may occur to you without my mention of them—but the one which arrested my attention was a sort of mental inertia, a dislike of change and of the labor of rearranging old ideas to meet the new conditions consequent on new advances. It was argued too that the laboratories were often misleading and that discoveries were put forward for general use long before they had been tested and retested with the caution that the case demanded. Reference was made to the famous instance of tuberculin, for which Koch appears to have been really less responsible than those who at the time dictated his utterances. The criticism is, however, in a measure just. I am painfully aware that in the laboratory a remoteness from real life sometimes weakens the sense of responsibility for results which are put forth, but these last decades, and especially the very last, have shown a vast improvement in this relation and the cooperation between the clinic and the laboratory has become most happily intimate.

I have spoken of the laboratory because it is an important source of knowledge for the clinician, though most naturally farthest removed from his daily experience. I ask you to remember that one may help medicine and yet do it as a chemist, a botanist or a zoologist—quite detached from the clinical applications of what is found. To grow pathologic organisms is a biological problem; to follow insect borne diseases takes one into entomology. The applications to medicine are incidental, but often of the greatest import. Remote then may be the sources of facts important for the practitioner, but, although they are remote, these sources should be neither forgotten nor neglected.

Though your knowledge is the best at the moment, yet to-morrow may see a change for the better by the introduction of some advance based on what is now an incidental laboratory test or clinical observation—yet to be applied. For the protection of this laboratory work not only your interest but your moral and professional support is needed.

I have dwelt on the fact that the patient is a different individual at different ages, and that your knowledge and ideas must change and grow with the continued pondering of experience. In that connection there is just one word to add. It touches growth in the physician himself—a very vital matter.

The intelligence tests about which much controversy has been waged during the past ten years have come to stay. They sometimes are disquieting. It is said that an intelligence of nine years suffices to rear and bring up a child. I do not know just what mental age admits one to the laboratory. Though further applications to the problems before you are

not needed here, yet I do want to point out that these tests show in a somewhat precise way a fact that can hardly have failed to strike us all, namely, that our associates stop growing mentally at quite different ages, some continuing to grow long after others have reached their limit.

When Cato learned Greek at eighty years it indicated more than that the farm was doing well and he had time to spare. It indicated a capacity for new interests and a mental retentiveness which are among the virtues of the youthful mind.

These are endowments which we would all desire but which are unevenly possessed. However, in any discussion which involves the problem of nature versus nurture, it can always be pointed out that whatsoever nature has or has not done, there still remains the possibility of modifying nurture—or the environment—and these possible modifications are worth attention.

Observation shows us that the differences between men are small, but that, small as they may be, they amount to a great deal. Slight improvement is worth a struggle and repays the effort. I am commending to you therefore the effort to keep growing mentally.

A remarkable example is that of Helmholtz. Helmholtz began as a physician—a very mediocre practitioner they say—but under the inspiration of Johannes Müller he became interested in physiology. This branch he followed by the study of the eye and ear, leading to his great works on physiological optics and sensations of tone—by the way devising that important instrument the ophthalmoscope. But the physiology of the sense organs called for physics. So well did he follow this lead that he became one of the first physicists, and linked his name with the doctrine of the conservation of energy. Still going forward he developed his mathematics and became eminent in that field also. Here is a steady growth through a long life. A great intellectual engine at his command was applied to field after field in succession, and always with a resulting advance in knowledge.

Such men set the pace and these pacemakers are the most helpful members of our race, for while those who have stopped growing have but a single response, "It can't be done," the pace-makers do it.

Naturally you ask what is the formula, how is it accomplished? Let me reply by a question: What do you think about when you are not working? For most of us that period represents the larger part of life, and it does make a difference what we do with this great fraction, so I will leave the implications of my question without elaboration, but ask you to meditate upon it.

There is however a further matter which lies closer at hand. Let us consider the "Bohemian." I mean the individual who bears this name by virtue of his behavior. He is worthy of attention. He protests against the restriction of conventions, sometimes in a not too seemly manner, but at his best with the hope of getting free from conditions which hamper thought or work. We all suffer from these restrictions in a mild way. By all accounts the savage seems to be most completely surrounded by taboos and conventional restraint. His is not a happy lot. Civilized man suffers less, and yet conventions stand in our way.

The necessity of getting back for dinner cuts into an experiment. The idea that one retires at a certain hour limits a series of observations. Very trifling these conditions, you will say, yet breaking life up into small lengths in a way which often interferes. War teaches us something here that may be useful.

In this connection I love the story of von Baer. Von Baer, the embryologist, tells how he went into his laboratory when the leaves were falling in the autumn and came out again when the spring flowers were in bloom. That was a day's work that counted, and we can do well to ponder on it. Von Baer was the sort of Bohemian I have in mind.

Here I rest my case.

The past four years have meant great things for medicine. For the first time in history the fighting man has had the best that medicine could give. Certain forms of practise have advanced by leaps and bounds, and you

come into action when medicine still feels the impulse of these strenuous years.

The laboratory and the clinic have collaborated as never before and the future is full of promise.

Under these conditions it has been my privilege to give you encouragement and a bit of counsel, and I feel indebted for the opportunity. HENRY H. DONALDSON

WILLIAM ERSKINE KELLICOTT

A CONSTANTLY lengthening list of scientific men who have surrendered their lives in varied war services, or in that harder, more exacting fight with microbial enemies, is one of those news columns which our eyes have come to scan with a strange mingling of suspense and unwilling, silent complacency. The world, and each of us in it, has become immeasurably poorer because of this great drain upon potential mental energy; and the lost men, as a rule, have had capacities for friendship directly commensurate with their intellectual powers.

Not a few American zoologists were particularly moved by a recent item of this sort; and to the list we are now compelled to append the name of William Erskine Kellicott, who was taken away by pneumonia, after illness of a week, at his home, in Hastings-on-Hudson, N. Y., January 29, 1919. Though but forty years of age, he, among scientists, teachers, critics and friends, had become to many their great, to some their greatest, satisfaction.

His career may be briefly summarized as follows: he was born in Buffalo, N. Y., April 5, 1878, the son of David Simmons Kellicott and Valeria Erskine Stowell. His father, at that time, was head of the science department in the Buffalo State Normal School. His earlier educational training was received entirely at home, so that he began his high school studies, at the age of twelve years, directly from his mother's tuition. This occurred at Columbus, Ohio, the second year of his father's appointment to the chair of biology in Ohio State University. After completion of his high school course, he entered the university, from which he received the degree of Ph.B. in 1898, with election to Sigma Xi. Later, on organization of a chapter of Phi Beta Kappa at Ohio State, he was chosen to that society also.

His undergraduate work was shaped and pursued with entire reference to a future career in surgery; but his father's death in his senior year changed this cherished plan, and he spent his first post-graduate year in teaching biological subjects in the high school at Marysville, Ohio. The following summer he was a student in the invertebrate zoology course at the Marine Biological Laboratory, Woods Hole, Mass., and it was at this time that Kellicott decided to devote his energies to zoological science. In the autumn of 1899 be began graduate study at Columbia University, and received the doctorate in 1904, his major thesis being entitled "The Development of the Vascular and Respiratory Systems of *Ceratodus*."

The following positions were occupied by him for the term of years indicated:

In Barnard College, assistant in zoology, '01–'02; tutor, '02–'05; instructor, '05–'06.

In Goucher College, professor of biology, '06–'18.

In College of the City of New York, professor of biology, '18–.

In the Marine Biological Laboratory, instructor in embryology, '11, '12, '14; in charge of the embryology course, '15–.

For the year 1912–13 he was fellow of the Kahn Foundation for the Foreign Travel of American Teachers, and as such was enabled to visit many European countries and numerous centers of interest in Siberia, China, Japan and India. His report to the foundation offers interesting proof of his discriminating analysis of human nature.

In July, 1918, he resigned as assistant statistician of the U. S. Food Administration, having served one year; during this time he devised and put into operation a thorough and efficient system of gathering data from dealers in food all over the country, definitely stamping the square dealer and the profiteer.

He was a fellow of the American Association for the Advancement of Science, a member of the American Society of Zoologists, of the American Naturalists, and of the New York Academy of Sciences.

On September 11, 1901, he was married to Mary Chappel Hicks, of Columbus, Ohio. Their daughter, Janet, fourteen, is now busy with her high school studies.

Not taking into consideration the devotion and thoughtfulness which characterized his home life, the main enthusiasm of this man was in the field of science; and this for the simple reason that he could tolerate nothing except truth. Keenly appreciative of language and literature, still he felt them to be of special value as being a means of giving expression to some sort or phase of truth. As an investigator he very sharply discriminated between the significant and the pointless, a clear, long perspective stretching out before the former, while the latter was given little patience. Kellicott had not chosen a particular problem as his special zoological interest; his research contributed to our knowledge of cytology, normal embryology, correlation, growth measurements, animal breeding and factors influencing development. A second paper dealing with the last-named question was in process of writing at the time of his death. He often reprimanded himself for thus not concentrating his investigative effort, and he doubtless would have selected a special field ere long; but ever insistent with him was the conviction that he must school himself in the current zoological movements of the day, that he might be the better trained and speak and think out of his own experiences. Exacting, though always kindly, in his teaching, he prescribed an even greater degree of discipline for himself. Assumption was seldom a mental experience with him. The following quotation is one of his own selection—"Surely, if there is any knowledge which is of most worth, it is knowledge of the ways by which anything is entitled to be called *knowledge,* instead of being mere *opinion,* or *guesswork,* or *dogma*" (Dewey).

As a teacher Kellicott instinctively knew the art of making subject matter appeal because of its own intrinsic significance; he did not obscure it by obtruding mannerisms or his own personality. Seldom is a man given a greater degree of loyalty by his students, or for better reasons, than was he. As a participant in administrative matters, he was broad-minded, simultaneously unafraid and cooperating, independent of precedent and practise where these seemed wasteful or obstructive. His influence seemed uniformly disproportionate to the length of his service and his academic title.

Kellicott's nature was too large to permit expression in one field alone. It was magnetically drawn toward the beautiful in music, in art, in the sculpture and adornment of nature's earth, and in human nature. His capacity for friendship was exceptional; companions of his own age felt themselves rich in the resources which were his; his seniors, startled by his passing, have become aware of how large a place he occupied in their confidence. One of them has written: "I didn't really know how much I loved the lad. I had formed the habit, unconscious till now, of thinking to myself, 'How would that strike Kellicott?'"

Side by side with his straight directness in thought and action, there dwelt a subtle, copious humor, an unstinted unselfishness and generosity, a buoyant gladness, which, as he "dwelt by the side of the road" of human lives, made him, in uncommon degree, "a friend to man."

It is better, and more just, that we do not circumscribe and limit the loss which has come upon science, the teaching profession, and upon his widening circle of friends by attempting to define in words the significance of the death of William Erskine Kellicott. "He is so vivid a man that he defends himself in your own mind against misinterpretation."

ROBERT A. BUDINGTON

SCIENTIFIC EVENTS

THE DIRECTORSHIP OF THE BRITISH NATURAL HISTORY MUSEUM

SIR LAZARUS FLETCHER retired on March 3 from the directorship of the Natural History Museum after forty-one years in its service. Previous to his appointment as director in 1909, he had served two years as assistant and twenty-nine years as keeper in the Mineral Department. In connecton with the appointment of his successor *Nature* prints the follow-

ing letter signed by twenty-three distinguished naturalists:

The director of the British Museum (Natural History) is about to retire, and we learn with deep apprehension that the principal trustees, with whom the appointment rests, have received, or are about to receive, from the general body of trustees a recommendation to pass over the claims of scientific men and to appoint a lay official, who is at present assistant secretary. The former directors, Sir Richard Owen, Sir William Flower, and Sir Ray Lankester, like the present director, Sir Lazarus Fletcher, were all distinguished scientific men. The Natural History Museum is a scientific institution. There is a large staff of scientific keepers and assistants. The director has to represent natural history to the public, to other scientific institutions at home, in the dominions and colonies, and in foreign countries, and to the many government departments with which the museum has relations. He must represent it with knowledge and authority. There are few posts with such possibilities of advancing the natural history sciences, of making them useful to the nation and of interpreting them to the public. The existence of the post is a great stimulus to the zeal and ambition of zoologists and geologists.

The arguments alleged in favor of the recommendation are trivial. It is stated that a former director was allowed by the trustees to leave the administrative details to the member of the clerical staff whom it is proposed to promote, that he performed these duties with ability, and during the tenure of the present director retained and extended his powers. It is urged that the tenure of the new director would be short, as he would have to retire in two years under the age limit. It is pleaded that promotion would entitle him to a larger pension, and that he need not be called director, but only acting-director.

Plainly, if the assistant secretary be the only man who knows the details of administration, it is important that the permanent director should be appointed at once, in order to have the opportunity of learning them before taking them over. In actual fact there is nothing in the administrative work of the directorship that could not be learned in a few weeks or months by any person of ordinary intelligence. At least two of the present keepers are eligible for the vacancy, have attained the necessary scientific standing, and have ample experience of the museum itself. To pass over these or several eminent and eligible men not on the staff in favor of one of the ordinary office staff would be an affront to scientific men and of grave detriment to science.

THE INYO RANGE AND THE MOUNT WHITNEY REGION

The Inyo Range, the Mount Whitney region and Owens Valley, which lies between these two ranges, in eastern California, are described in a report just issued by the United States Geological Survey, as Professional Paper 110 by Adolf Knopf. This region is off the main lines of travel and is not so well known as other parts of the state, but when the roads and railway facilities are improved, Owens Valley, which affords the easiest access to the region, will certainly become famous for its magnificent scenery. The Sierra Nevada, which reaches its highest point in Mount Whitney, forms the west wall of Owens Valley, and as it rises abruptly above the valley without intervening foothills the range displays its majestic height far more imposingly here than anywhere else along its course. The top of the Sierra Nevada is readily accessible by trails that start from the pleasant towns of Lone Pine, Independence, Big Pine and Bishop. Good roads extend into the heart of the range from Bishop, the chief town in Owens Valley, so that an automobile trip of hardly more than an hour will take the traveler to the headwaters of Bishop Creek, whose profoundly glaciated canyons and spacious amphitheaters are among the most impressive in the entire range. The country west of the crest of this part of the Sierra Nevada is included in the proposed Roosevelt National Park.

The region is rich in mineral resources—silver, lead, zinc, tungsten, gold and marble—and the waters of Owens Lake yield soda and other chemicals. The mines at Cerro Gordo, in the Inyo Range, have produced more silver-lead ore than any other mine in California, their output of base bullion between 1869 and 1877 amounting to $7,000,000. After those early flush times the mines long lay idle, but in recent years they have been reopened, and Cerro Gordo has again become California's foremost producer of lead ore.

In 1913 large bodies of tungsten ore were discovered in the Tungsten Hills, west of Bishop. They remained practically unknown until the spring of 1916, when outside interests bought them and began to develop them energetically. By midsummer two mills had been completed and were in active operation, and the district has since supplied a large quantity of tungsten. Geologic conditions similar to those in the Tungsten Hills prevail over a wide extent of country along the east slope of the Sierra Nevada. The places of contact of the intrusive granites with other rock, shown in the geologic maps accompanying the paper, are the most likely places to prospect for other similar bodies of tungsten ore.

THE JOURNAL OF "NATURAL HISTORY"

THE *Journal of the American Museum of Natural History* will hereafter be known as *Natural History,* being edited as hitherto by Miss Mary Cynthia Dickerson, curator of woods and forestry. The change is announced as follows:

Attention is called to the change in title of this magazine from *American Museum Journal* to the old, honorable and historical name *Natural History.* A change has been contemplated for two years or more, partly to avoid confusion with other publications known as "Museum Journals" and partly because the magazine for these years has not restricted itself to a consideration of the American Museum's work and interests. As expressed many times by the editor in letters to contributors, the magazine would like to feel that it stands as a medium of expression between authoritative science in America and the people, a place for publication of readable articles on the results of the scientific research and thought of the nation for people who are not technically trained. These people have neither time nor desire to pore over technical, unreadable articles, but nevertheless are intelligently, practically and often profoundly interested. *Natural History* would like to stand for the highest type of authoritative natural history, expressed by the investigators themselves, by explorers, by the accurate observers in laboratory or field. In addition it desires to interpret the technical publications of our scientific thinkers, if not by popular articles by the same authors, then through reviews by other well-known scientific thinkers, these "reviews" being, as suggested, readable discussions of the given subject apropos of the technical work. It would also of course report phases of the educational work being accomplished by the scientific departments of the United States government and by the various scientific institutions of the country, especially those of the museum type.

There has been so much shallow, inaccurate, "popular" science, nature study and natural history, written by persons untrained in science and with distorted imaginations, that a prejudice still remains in the minds of some scientists against putting their observations and conclusions, even when of great value for the layman, into readable form. But the time of such suspicion and condemnation against the mere form of expression of an idea is well-nigh past, and the greatest scientific men of the country are daily proving their willingness and desire to write in a way to be understood not only by the trained technical man, but also by the man with no knowledge of the shorthand of the scientific vocabulary.

We need especially to have a knowledge of nature and science to-day. The day of necessity has come for conservation of the world's natural resources and preservation of animals fast becoming extinct; there is seen approaching the time of conscious control of evolution; and just ordinary culture demands in the present decade knowledge of science in addition to what it has always demanded in literature, music and art. And these reasons do not take account of the added joy in life that comes from a knowledge of nature. We people of to-day need to know the book of the earth, to study it as a Bible, feeling the divinity in it. *Natural History* hopes to meet this need in part.

DEGREES IN PUBLIC HEALTH

IN view of the importance of arriving at some measure of standardization for the various degrees and certificates offered in the field of public health, Yale University invited a group of representatives from neighboring universities to confer in regard to the matter at New Haven on February 28, 1919. Johns Hopkins University was represented by Dr. W. H. Welch, the Massachusetts Institute of Technology by Professor W. T. Sedgwick, Harvard University by Dr. M. J. Rosenau, New York University by W. H. Park, and the University of Pennsylvania by Dr. H. F. Smyth; while Yale University was repre-

sented by a special committee from the Graduate School, including Professor S. E. Barney. Professor L. B. Mendel, Professor L. F. Winternitz and Professor C.-E. A. Winslow.

After very full discussion of the various points involved the following resolutions were unanimously adopted:

1. That the degree of Doctor of Public Health (for which the abbreviation should be Dr.P.H.) for graduates in medicine should normally be awarded after two years of work done under academic direction, of which one year at least should be in residence; and that the requirements for the degree should include class work, practical field work, and an essay based on individual study of a particular problem.

2. That the degree of Doctor of Philosophy or Doctor of Science in Public Health or Hygiene should be conferred upon students who hold the bachelor's degree from a college or technical school of recognized standing, and have satisfactorily completed not less than three years of graduate study. It is understood that this degree is based upon the fundamental sciences associated with hygiene and public health, including a knowledge of physics, chemistry, general biology, anatomy, physiology, physiological chemistry, pathology and bacteriology, in addition to the thesis and other usual requirements for the Ph.D .or Sc.D degree.

3. That the Certificate in Public Health should be granted for not less than one academic year of work to those who have received a bachelor's degree from a recognized college or technical school, or have satisfactorily completed two years of work in a recognized medical school, provided they have previously pursued satisfactory courses in physics, chemistry, general biology and general bacteriology.

4. That the degree of Bachelor of Science in Public Health or Hygiene should be given for the completion of a four years course, the last two years of which have been devoted to the fundamental sciences associated with hygiene and public health.

5. That the authorities having the appointment of health officials be urged to give preference so far as possible to persons holding degrees or certificates in public health or hygiene.

SCIENTIFIC NOTES AND NEWS

Sir J. J. Thomson has expressed his desire to resign the Cavendish professorship of experimental physics at the University of Cambridge, but has offered to continue his services in the promotion and direction of research work in physics without stipend.

Colonel William H. Welch, of the Johns Hopkins University, has sailed for France, where he will attend the health conference of the International Red Cross.

The following fifteen candidates have been nominated by the council of the Royal Society for election into the society: Professor F. A. Bainbridge, Dr. G. Barger, Dr. S. Chapman, Sir C. F. Close, Dr. J. W. Evans, Sir Maurice Fitzmaurice, Dr. G. S. Graham-Smith, Mr. E. Heron-Allen, Dr. W. D. Matthew, Dr. C. G. Seligman, Professor B. D. Steele, Major G. I. Taylor, Professor G. N. Watson, Dr. J. C. Willis and Professor T. B. Wood.

Fellows of the Royal Society of Edinburgh have been elected as follows: Dr. A. R. Cushny, Dr. W. J. Dundus, Dr. R. O. Morris, Dr. T. S. Patterson, Mr. B. D. Porritt, Mr. A. H. Roberts, Mr. W. A. Robertson, Dr. A. Scott, Dr. A. R. Scott, Mr. W. W. Smith and Captain D. A. Stevenson.

Lieutenant General Sir Charles H. Burtchaell, K.C.B., director-general of the British Army Medical Service in France, has received the honorary degree of LL.D. of the University of Dublin, from which he graduated in 1889. He has also received the honorary fellowship of the Royal College of Surgeons in Ireland.

Dr. William K. Gregory, associate in paleontology in the American Museum of Natural History, New York, was recently elected a corresponding member of the Zoological Society of London.

Major John W. Churchman, M.R.C., professor of surgery at Yale University, has been named "Officier de l'Instruction Publique" by the French government in recognition of his services as Medicin chef of Hospital militaire 32bis, Passy, France. during 1916.

Dr. M. G. Seelig, professor of surgery in the medical school of St. Louis University, has received his honorable discharge from the

Army, and has accepted the commission of colonel in the Medical Section of the Officers' Reserve Corps.

AMONG the members of the University of California faculty who have been on war leave and have now returned to their work at the university are: W. B. Herms, associate professor of parasitology; G. R. Stewart, assistant professor of agricultural chemistry; A. W. Christie, instructor in agricultural chemistry, and W. D. Norton, H. E. Drobish and F. T. Murphy, assistants in agricultural extension.

THE laboratory of forest pathology of the Bureau of Plant Industry, U. S. Department of Agriculture, formerly located at Missoula, Montana, has been permanently established in Spokane, Washington, with Dr. James R. Weir in charge.

DR. H. M. HALL, who recently resigned as associate professor of economic botany in the University of California, has accepted a position with the Carnegie Institution of Washington. The vacancy in the department of botany at Berkeley has been filled by the appointment of Dr. F. J. Smiley, who has been associate professor of botany and geology in Occidental College.

MR. C. D. SHANE has been appointed assistant in the Lick Observatory, University of California.

DR. LEE R. DICE (California, 1915), formerly zoologist of the Experiment Station and instructor in zoology at Kansas State Agricultural College, and later assistant professor of biology at Montana State University, has accepted the position of curator of mammals in the Museum of Zoology, University of Michigan.

MR. CHARLES HOWARD RICHARDSON, recently a research chemist with the Röhm and Haas Chemical Company, Bristol, Pa., has been appointed specialist in insect physiology, Bureau of Entomology, Washington, D. C.

DR. CHESTER N. MYERS, organic chemist of the Hygienic Laboratory, Public Health Service, has resigned to organize a research laboratory for H. A. Metz and Company, New York.

DR. H. J. SPINDEN, of the anthropology department of the American Museum, has returned from an archeological and ethnological expedition to Central America and Columbia.

A COAST and Geodetic Survey party, under the direction of O. W. Swainson, is at work on the triangulation and topographic surveying of the Virgin Islands, recently acquired from Denmark.

PROFESSOR L. C. GRATON, who has been in New York as secretary of the Copper Producers' Committee, one of the few war committees for industrial control organized and administered by the concerned industry itself though acting under authority of the War Industries Board, will soon return to Harvard University to take up the work in mining geology and to revive the secondary enrichment investigation. Before leaving New York, he will repeat at Columbia the series of lectures he gave there last year on oxidation and secondary enrichment.

MR. GERALD H. THAYER gave an illustrated lecture on "Camouflage and Protective Animal Coloration" at the New York State College of Forestry at Syracuse on March 18.

THE seventh Harvey Society lecture of the present series will be by Dr. Stewart Paton, of Princeton University, formerly major, U. S. A., on "Human Behavior in War and Peace" at the New York Academy of Medicine on Saturday evening, April 12.

ON March 25, an exhibition of motion pictures of plant life was held at the Brooklyn Botanic Garden under the joint auspices of the Torrey Botanical Club and the Botanic Garden. Among the subjects illustrated were the penetration of the tissue of a potato tuber by the hypha of the parasitic fungus that causes the potato leak disease, and bridge grafting to save fruit trees which have been girdled by rodents or otherwise. The films were explained by Dr. R. B. Harvey, of the Department of Agriculture.

DR. LEONARD HILL, F.R.S., delivered a lecture on the atmospheric conditions which affect health, before the Royal Meteorological Society on March 19, in the lecture room of the Geo-

logical Society. The chair was taken by Sir Napier Shaw, F.R.S.

A MEMORIAL service in honor of the late President C. R. Van Hise will be held by University of Wisconsin late in April, according to plans now in preparation. The speakers will be: Professor T. C. Chamberlin, of the University of Chicago, who will speak on Dr. Van Hise's relation to science; Dr. Albert Shaw, of *The Review of Reviews,* who will speak on Dr. Van Hise's relation to the public, and President E. A. Brige, of the university, who will speak on Dr. Van Hise's relation to the university.

DR. FREDERICK DU CANE GODMAN, the distinguished English naturalist, died on February 19.

J. J. T. SCHLŒSING, professor of agricultural chemistry in the Paris Conservatoire des Arts et Métiers, died on February 8 at the age of ninety-four years.

ANDRÉ CHANTEMESSE, professor of hygiene in the Paris faculty of medicine and inspector general of sanitary services, has died in his sixty-fifth year.

ANDREW MELVILLE PATERSON, professor of anatomy at the University of Liverpool, died on February 13, aged fifty-six years.

THE British Association for the Advancement of Science will resume its series of annual meetings this year at Bournemouth from September 9 to 13, under the presidency of the Hon. Sir Charles Parsons.

A CONSIDERABLE fund has been given to Montefiore Home and Hospital, Gun Hill Road, New York City, the income of which is to be devoted to medical research independent of the hospital laboratory work. The selection of a director of research is at present under consideration and is in the hands of the laboratory committee.

THE ninth session of the Marine Biological Laboratory at Laguna Beach, California, will begin on June 25 and last six weeks. General courses in marine zoology, botany and entomology will be given. There are eight small private rooms for the use of special investigators. Copies of the announcement may be obtained by writing to the Department of Zoology, Pomona College, Claremont, California.

UNIVERSITY AND EDUCATIONAL NEWS

DR. J. B. HURRY has offered to increase the value of the Michael Foster research studentship in physiology, founded by him at the University of Cambridge in 1912, and tenable biennially, from a hundred guineas to £200.

DR. WITHROW MORSE, of the Michael Reese Hospital at Chicago, has received an appointment as professor of physiological chemistry in the medical school of the University of West Virginia, Morgantown.

DR. EUGENE L. PORTER, instructor in physiology in the University of Chicago, has been appointed assistant professor of physiology at the Western Reserve University Medical School.

DR. FRANK J. SMILEY, has been appointed assistant professor of economic botany and assistant botanist in the Agricultural Experiment Station, University of California.

THE Manchester City Council has approved the appointment of Arthur James Turner, to the chair of textile technology in the College of Technology, Manchester. Professor Turner will be assisted in conducting the work of his department by colleagues who are practical experts in various branches of the textile industries.

DISCUSSION AND CORRESPONDENCE

"OLD AGE" OF CHEMICAL ELEMENTS

TO THE EDITOR OF SCIENCE: In his presidential address Professor Richards discussed the interesting problem of radioactive lead.[1] The present conception regarding the structure of atoms and the theory of electrons seems to indicate that the chemical elements may be subject to "old age." According to this hypothesis "common" lead possessed eons ago, probably at the time of the formation of the earth, an atomic weight of 206.08 and density of 11.27 and by slowing up in the speed of its electrons increased its

[1] SCIENCE, 49, p. 1, 1919.

atomic weight to 207.19 and density to 11.34. Similarly "neo" lead, or freshly created lead, formed by radio-active disintegration, would very slowly increase its mass and eons hence have a higher atomic weight and density.

All other elements should be subject to an increase in mass and it could therefore be predicted that *e. g.*, helium of the atmosphere and of minerals will have an "atomic weight" which is .0214 higher than the atomic weight of helium from radioactive disintegration, that is: there should be an "old" or "common" helium with atomic weight of about 3.94, and a "neo" helium of atomic weight 3.92.

There is at present no evidence for the fallacy of such a speculation, in fact it seems to be supported by certain widely different phenomena to wit:

(*a*) *Radio-activity.*—The difference in the atomic weights of "leads" is explained in a plausible way. It is a safe guess to predict that common lead can not be separated. While a mixture of common and radioactive lead might be separated by fractional diffusion.

(*b*) *Astro-physics.*—The stellar evolution, revealed by characteristic types of stellar and nebular spectra, points to a close relationship between the constituents of celestial bodies and the periodic law. This indicates an evolution of chemical elements following the periodic system.

(*c*) *Geo-chemistry.*—Here the remarkable fact is shown that over 99 per cent. of the elements upon the known earth surface are those of low atomic weight. These elements occupy neighboring places in the periodic system which seem to indicate that the earth has reached a certain definite stage of evolution, practically halfway of the third period in the periodic system.

(*d*) *Bio-chemistry.*—From 96–99.5 per cent. of all living matter is composed of the four elements C, H, O and N, all four being neighbors in the periodic system. The other elements essential to life are closely grouped around.

Therefore is it not possible that biological evolution follows stellar evolution, and stellar evolution follows chemical evolution? If stellar and chemical evolution go hand in hand, then the physical, chemical and biological condition of a celestial body will depend entirely upon its age.

Where is the evidence that the elements of to-day were eons ago the same substances and preserved their properties unaltered? It is possible that the electrons of the atom might very slowly lessen their orbital motion and thereby attract and hold additional free electrons thus increasing in valency and mass. Thus, *e. g.*, a sodium-atom by catching an electron would increase in its valency and become a magnesium-atom. Magnesium in time transmutates into aluminum and so on.

Just as the astronomical experience of mankind is recognized to be a snapshot of the universe, so all chemical and physical knowledge of man is the limited inventory, taken during an infinitesimal fraction of eternity.

INGO W. D. HACKH

BERKELEY, CAL.,
January 22, 1919

DESICCATED VEGETABLES

AN admirable exposition of the anhydrous food industry has recently appeared in a government bulletin entitled "Relation of Dehydration to Agriculture" and written by Major S. C. Prescott. After reading this paper one can not fail to come to the conclusion that the preparation of dried foods is destined to become a very important industry. However, before such an industry can yield the maximum return it is essential that the eaters of the dried foods be scientifically assured that desiccated foods possess proper nutritive value. Some such investigations have been made but there is a need for very comprehensive studies.

It is a matter of common knowledge that desiccated vegetables, for example, will assume a form closely approaching that of the fresh vegetable after having been immersed in water for a few hours. This fact is often cited as demonstrating that there has been no alteration in the structure of the vegetable cell during the dehydration process. However, if we

remove the swollen vegetable from the water and permit it to remain at room temperature for twenty-four to thirty-six hours *it will return to its anhydrous state*. This phenomenon, it seems to me, stamps the anhydrous product as an entirely different product, structurally, from the fresh product, but does not necessarily indicate any lowering in food value. In other words a fresh vegetable holds its water much more tenaciously than does a dehydrated vegetable which has had its water removed and has subsequently been immersed in water and made to assume a form closely approximating that of the fresh vegetable. Is the failure of the anhydrous vegetable to retain its water to the same degree as the fresh vegetable due to the fact that the drying has brought about some change in the colloids of the vegetable cells which lowers their power to hold water? Or does the removal of salts through the "soaking" process lower the imbibition power of the colloids? Or is there some other answer? An explanation from our friends the physical chemists would be in order.

The above phenomenon was called to my attention by Mr. Charles Denby of the War Trade Board and Mr. Daniel Moreau Barringer both of whom are much interested in the general problem of food desiccation.

Philip B. Hawk
Jefferson Medical College,
Philadelphia

NONSILVERABLE CONTAINERS FOR SILVERING MIRRORS

To the Editor of Science: In connection with recent contributions to your columns under the title "Nonsilverable Containers for Silvering Mirrors" the writer may be permitted to record an observation made several years ago. This was that silvering solution could not be made to deposit on black amorphous selenium, although it coated the walls of the glass vessel in which the piece of selenium was placed. The converse of this experiment, namely silvering a piece of glass in a vessel lined with selenium, was not tried, but would appear to offer the solution of the problem of a container that will not attract silver.

Herbert E. Ives

AD REM OF A HISTORY OF SCIENCES IN THE UNITED STATES

In the long years of my labors in scientific reference work I found myself greatly hampered by the lack of an available source history of the different branches of sciences, especially of the exact sciences, in the United States. There are three important contributions in this field, all written by the late George Brown Goode: "The Origin of the National Scientific and Educational Institutions of the United States," 1890; "The Beginnings of Natural History in America," 1886; and "The Beginnings of American Science," 1887. Nobody who is acquainted with these papers can withhold his admiration for Mr. Goode's painstaking work, but after all they are only stepping stones and cover only a limited period, and serve merely, as it was contemplated by Mr. Goode, as an outline.

The more interested I became in the matter the more I found myself impressed by the idea to see that this great lacuna should be filled. The best channel through which to accomplish this seemed to me to lay the matter before the American Association for the Advancement of Science, have it discussed there in its entirety, and if possible undertaken by the association or under the auspices of the association. The outbreak of the world war made it seem advisable to me to postpone my plan. A year or two ago I broached the subject with Dr. L. O. Howard, the permanent secretary of the association, who fell in with the idea and expressed his willingness to submit my suggestion to the committee on policy, whenever I should be ready to present it in concrete form. Last October when the end of the war seemed to be only a matter of months I thought the time had come for action. Therefore, I addressed on October 25th the following communication to Dr. L. O. Howard:

Dear Sir:

There is as yet no history of sciences in the United States showing the important and far-reaching participation of our men of science in the general development of science. Now seems to be the proper time to seriously consider such an undertaking, as the great world war has changed and

will change not only political history but science to such an extent, that a genetic and historical survey is imperative. Therefore, in my opinion, the period to be dealt with should begin with the earliest original contributions of American men of science to the different fields of knowledge and close with the beginning of the world war. What is achieved during the world war marks a beginning of a new epoch in our national scientific life and should be treated later on.

As the American Association for the Advancement of Science is the representative scientific body of the United States, it is only proper that the history of sciences in the United States should be undertaken and edited under the auspices of the association.

Therefore, I beg to ask you as permanent secretary of the association to submit this proposition at the next meeting of the association and have it voted upon.

If the association should vote in favor of the motion the next step would be to consider how to proceed in this matter. In regard to this I wish to make the following suggestions, either of which should be carried out.

The first one would be to appoint a historical committee for this special purpose, in which each section of the association should be represented by one member, in addition to which three members at large should be appointed by the president. The president and the permanent secretary should be members of this committee ex-officio. The president acts as presiding officer of the committee, but might delegate any member of the committee as acting chairman.

The second suggestion would be to add a new section called "The Historical Section," and let them formulate a plan and submit it to the association. This section should be a permanent one, having the same organization as the other sections. Its purpose should be to promote the study of the history of sciences in the United States.

This second suggestion should be voted on even if my plan for a history of sciences in the United States undertaken under the auspices of the Association should be vetoed.

In a personal interview Dr. Howard kindly informed me that he would submit my letter to the committee on policy which was to meet in November in Baltimore. As several members of the committee were unable to attend, the meeting was not held and the question came up before the committee on policy during the recent meeting of the association. At this meeting of the committee it was decided to propose, among others, the following change in the constitution: that a new section, called section K:[1] "Historical Science" be formed. This proposed change is to be voted on at the next meeting of the association in St. Louis, December, 1919.

I sincerely hope that the association will vote in favor of it. I want to raise only one objection, and that is the designation of the proposed section. "Historical science" seems to me not very appropriate and really covers an entirely different subject, or is at least open to doubt. In my opinion the wording "historical science" would rather refer to history as science, which is a cultural science, while the new section "K" should deal with the history of the different branches of exact sciences. In my suggestion submitted to the association I proposed as the name of the new section: "Historical Section." An afterthought shows me that this designation may be subjected to the same criticism. Therefore, I propose now as the proper designation of section "K" the name: "History of Science," which would express the contemplated work of the section without any doubt: "To promote the study of the history of sciences in the United States."

On the main point of my suggestion in regard to a "history of sciences in the United States undertaken and edited under the auspices of the American Association for the Advancement of Science," the committee has taken no action.

This gives me a certain liberty of action. I hope that this question may be aired at the next meeting at St. Louis; meanwhile I would like to bring the matter to the attention of our scientific men and institutions, and a discussion of the project in the columns of Science would be very welcome to me.

Felix Neumann

Army Medical Museum and Library,
Washington, D. C.

[1] Owing to a division of several sections a new lettering of the sections has to be adopted.

SCIENTIFIC BOOKS

Electroanalysis. By EDGAR F. SMITH. P. Blakiston's Son and Co., Philadelphia. 1918. Pp. 344, 47 figs.

This book is so well known that a brief mention of the fact that a new, sixth, edition of this national standard had appeared would suffice. Compared to the fifth edition which appeared in 1911, a number of additions to the text have been made, so that "in its present form there is presented the most recent and complete picture of the subject to which the book is devoted." "The book brings together all that has been found reliable, by the test of experience and offers simultaneously the latest results gathered in recent years from widely removed centers.' '

The first edition of the book appeared at Philadelphia in 1890 and comprised 116 pages. In reviewing the book at that time Ira Remsen wrote:

> Chemists will find this little book an excellent guide to a knowledge of the methods of quantitative analysis by electrolysis. As the author has himself contributed not a little to our knowledge of these methods, he is especially prepared for a work of this kind.

Up to 1890 text-books on analytical chemistry paid very little if any attention to electrolytic methods. The appearance of Dr. Smith's book marked the opening of a new era in quantitative analysis. The book was welcomed by chemists throughout the country and abroad and within a very few years thousands of determinations were annually made by electrolytic methods.

In 1901 Smith and his students, notably F. F. Exner, introduced the rotating anode with high currents and high voltages. Determinations which had formerly required two to four hours were reduced to five to ten minutes —and the quantities that could be accurately determined were more than threefold the quantities by the older methods.

The adoption of electroanalytic methods has been so rapid and so widespread that to-day it is hard to find a laboratory that does not include a complete outfit for electrolytic determinations. Apparatus builders are now placing upon the market standard equipments, saving much time and expense.

The wide scope of the book as it appears to-day is evident from the following brief summary of the contents: The first part of the book is devoted to the selection and description of suitable apparatus, a historical sketch and a very clear outline of the important underlying theories. The second or special part of the book (about 250 pp.) is devoted to the determination and separation of metals, halogens and nitric acid; the use of the mercury cathode; the electroanalysis of natural sulfides, arsenides, chromite, etc. Among the most recent additions to the text may be mentioned the paragraph on the use of Gooch's platinum-coated glass in place of solid platinum, the quantitative determination of cobalt as Co_3O_4 from ammonium fluoride-nitric acid solutions; and the description of the improved double mercury cup in which "hundreds of halides have been successfully analyzed."

One of the characteristic features of Smith's book as compared with other books on analytical chemistry, is the inclusion in the text of tables recording in detail actual experiments carried out according to the methods suggested. Of interest and value, furthermore, are the detailed literature references, a welcome guide to all who care to investigate the subject more fully. There is no attempt made to supply a "recipe" for every determination and every separation. Where accurate methods have not yet been propounded it is frankly acknowledged that such are lacking. Perhaps, too little space is devoted to the rotating cathode, used to advantage at times in place of the rotating anode. In a number of the large commercial laboratories the rotating cathode has been chosen in preference to the rotating anode on account of simplicity of mechanical layout.

The subject matter throughout the book is presented in so masterful a style that it can not fail to inspire confidence not only in the young student but also in the analyst of "many and varied experiences." To the student, the teacher and the analyst the book is an indispensable guide. As a chemical publication

Smith's "Electroanalysis" will always remain one of the American classics.

Colin G. Fink

S. Yonkers, N. Y.

REPORT OF THE COMMITTEE ON GENERIC TYPES OF THE BOTANICAL SOCIETY OF AMERICA

At the recent meeting of the Botanical Society of America at Baltimore the appended report was submitted. The proposed regulations for fixing generic types were accepted with the suggestion that they be published and distributed among botanists for their consideration. These regulations, being a part of a proposed Code of Nomenclature, should await the formulation of the latter for final adoption. The second part of the report, dealing with the Permanent Committee on Nomenclature, was adopted and the action recommended was authorized.

A. S. Hitchcock

Bureau of Plant Industry,
Washington, D. C.

REPORT OF THE COMMITTEE ON GENERIC TYPES

At its last meeting the society authorized the president to appoint a committee of three upon Generic Types. The members appointed were N. L. Britton, A. S. Hitchcock (chairman) and B. L. Robinson. Dr. Robinson declined to serve and it was found impracticable to obtain a representative from the Gray Herbarium. The remaining members, after some preliminary work, felt that it would be desirable to have the committee enlarged to represent a wider field of American botany. They therefore asked the incoming president, Dr. Trelease, to appoint J. M. Greenman as the third member of the committee and to add two other members, Leroy Abrams and Witmer Stone. The president felt that he did not have authority to enlarge the committee but suggested that the committee ask Messrs. Abrams and Stone to cooperate with it. This was done and these two have served on the committee as if they were members, and the report herewith submitted has received their approval. The committee, as now constituted, represents botanical institutions at Washington, New York, St. Louis, Philadelphia, and on the Pacific Coast.

The members of the committee were first asked to indicate their attitude toward the question of type species. Should the application of generic names be determined by type species; or should a generic name be applied to a generic concept independent of particular species?

The prevailing opinion being in favor of type species, there was sent to the members of the committee, a circular outlining the methods which might be used for selecting type species. Wishing to obtain advice and cooperation from competent botanists throughout the country the circular was sent to about fifty members of the Botanical Society.

It was overwhelmingly established that the botanists were in favor of the two fundamental principles: (1) The application of generic names shall be determined by type species; (2) The type species shall be the species or one of the species included in the genus when originally published (publication of genera of seed plants dating from the issue of Linnæus' "Species Plantarum" in 1753). In addition the opinion was prevailingly in favor of rules approaching those finally agreed upon by the committee.

Circular 5 contained a set of proposed regulations for fixing generic types and a few minor changes were made, resulting in the regulations as included in our report.

The committee makes two recommendations, (1) the adoption of a set of regulations for fixing generic types, and (2) the appointment of a permanent committee on nomenclature.

REGULATIONS FOR FIXING GENERIC TYPES.

INTRODUCTION

Rules of nomenclature should commend themselves as being reasonable and they should be as definite in their application as is consistent with reasonableness. In preparing the regulations the committee consulted other codes of nomenclature, the most important of which are the following:

The International Code of Zoological Nomenclature (see Treas. Dept. Hygien. Lab. Bull. 24 by C. W. Stiles; same with appendix and summaries of opinions 1–67, extracted from Proc. 9th Internat. Zool. Congr. 1913, published by T. O. Smallwood. Opinions 1–67 were published by the Smithsonian Institution). Article 30 concerns generic types.

The Code of Nomenclature adopted by the American Ornithologists' Union. Canons 21–24 concern generic types.

The Entomological Code. Banks and Caudell. Par. 93–106 concern generic types.

International Rules of Botanical Nomenclature. Vienna, 1905. The question of types is not touched upon. The application of generic names is considered in Arts. 45–46.

The American Code of Botanical Nomenclature (see *Bull. Torrey Club*, 34: 167, 1907). Canon 15 concerns generic types.

Recognizing the impossibility of framing a set of rules which shall cover all cases, since all contingencies can not be foreseen, the regulations have been divided into rules and recommendations. Under the rules are included statements of general principles to which all generic nomenclature should conform when considered from the standpoint of the type concept. It is thought that the types indicated by these rules will be acceptable without question to the great majority of botanists.

In order to adapt the genera of the older botanists to the modern concept to types it is necessary for us now to select type species for those genera for which no type would be indicated under our present rules. Some codes attempt by means of detailed rules automatically to select type species, hoping thus to secure uniformity, definiteness and stability. As all difficulties in the application can not be foreseen, the results in some cases have been confusing and have tended to cast disrepute upon the rules. This committee has appreciated the desirability of framing a code which shall possess definiteness but has endeavored to secure this by giving to a committee judicial functions.

The second part of the regulations consists of a series of recommendations. These are fairly elastic and can be applied reasonably rather than arbitrarily. In a large majority of cases the results obtained would be unquestioned. There would be, however, a small number of cases, especially among Linnæan genera, in which competent botanists might arrive at different results. It is proposed that such cases should be referred to a permanent committee which shall investigate them and recommend decisions to this society. It is believed that by this method types of genera may be selected which will receive the approval of the great majority of botanists. We look forward to an international agreement upon the types of all genera, thus laying the foundation for stability in nomenclature.

The proposed regulations follow:

I. RULES

Article 1. The application of generic names shall be determined by type species.

Article 2. The type species shall be the species or one of the species included in the genus when originally published (publication of the genera of seed plants dating from the issue of Linnæus's "Species Pantarum" in 1753).

(*a*) If a genus includes but one species when originally published, this species is the type.

Article 3. When, in the original publication of a genus, one of the species is definitely designated as type, this species shall be accepted as the type, regardless of other considerations.

(*a*) If *typicus* or *typus* is used as a new specific name for one of the species, this species shall be accepted as the type as if it were definitely designated.

Article 4. The publication of a new generic name as an avowed substitute for an earlier one does not change the type of the genus.

Article 5. If a genus, without an originally designated type, contains among its original species one with the generic name used as a specific name, either as a valid name or synonym, that species is to be accepted as the type.

Example.—The type species of Pentstemon

(Ait. Hort. Kew. 2: 360. 1789) is *Chelone Pentstemon* (L. Sp. Pl. 612. 1753; ed. 2. 850. 1763) because the later is cited as a synonym under one of the species of *Pentstemon*.

Article 6. If a genus, when originally published, includes more than one species, and no species is definitely designated as type, nor indicated according to Article 5, the choice of the type should accord with the following principles:

(*a*) Species inquirendae or species doubtfully referred to the genus, or mentioned as in any way exceptional are to be excluded from consideration in selecting the type.

(*b*) Genera of the first edition of Linnæus's "Species Plantarum" (1753) are usually typified through the citations given in the fifth edition of his "Genera Plantarum" (1754) except when inconsistent with the preceding articles.

Example.—Arundo (L. Sp. Pl. 81. 1753) is typified by *A. Donax* since this is the species figured by Scheuchzer in the plate cited by Linnæus (Gen. Pl. 35. 1754).

(*c*) Species which definitely disagree with the generic description (provided others agree), or which possess characters stated in the generic description as rare or unusual, are to be excluded from consideration in selecting the type.

II. RECOMMENDATIONS

Article 7. In the future it is recommended that authors of generic names definitely designate the type species; and that in the selection of types of genera previously published, but of which the type would not be indicated by the preceding articles, the following points be taken into consideration:

(*a*) The type species should usually be the species or one of the species which the author had chiefly in mind. This is often indicated by

1. A closer agreement with the generic description.

2. Certain species being figured (in the same work).

3. The specific name, such as *vulgaris, communis, medicinalis* or *officinalis*.

(*b*) The type species should usually be the one best known to the author. It may be assumed that an indigenous species (from the standpoint of the author), or an economic species, or one grown in a botanical garden and examined by the author, would usually represent an author's idea of a genus.

(*c*) In Linnæan genera the type should usually be chosen from those species included in the first technical use of the genus in pre-Linnæan literature.

Example.—The type species of *Andropogon* L. should be chosen from the two species included by Linnæus in the first use of the name (L. Fl. Leyd. 1740).

(*d*) The types of genera adopted through citations of non-binomial literature (with or without change of name) should usually be selected from those of the original species which received names in the first binomial publication.

Example.—Cypripedium (L. Sp. Pl. 951) is typified by *C. Calceolus*. Under *Cypripedium* (Gen. Pl. 408. 1754) Linnæus cites *Calceolus* Tourn. 249. Tournefort mentions 5 species, one of which is cited under *Cypripedium Calceolus* by Linnæus.

(*e*) The preceding conditions having been met, preference should be shown for a species which will retain the generic name in its most widely used sense, or for one which belongs to a division of the genus containing a larger number of species, or, especially in Linnæan genera, for the historically oldest species.

Example.—Phalaris L. is typified by *P. canariensis* because it is the only one of the 5 Linnæan species known to the older writers (such as Bauhin) by the name of *Phalaris*, so far as shown by the synonyms given by Linnæus.

(*f*) Among species equally eligible, the preference should be given to the first known to have been designated as the type.

(*g*) If it is impossible to select a type under the conditions mentioned above the first of equally eligible species should be chosen.

PERMANENT COMMITTEE ON NOMENCLATURE

1. It is recommended that the present committee be enlarged to 9 members and be made a standing committee on Botanical Nomencla-

ture, the two members who have acted in co-operation with the committee to be formally added to that body; and that the president of the Botanical Society appoint additional members, one with a special knowledge of the Bryophyta or Pteridophyta, one with a special knowledge of the Algæ, and two with a special knowledge of fungi.

2. This committee shall investigate doubtful or questioned cases, either upon its own initiative or in response to requests, and shall recommend decisions. It may prepare a code of Botanical Nomenclature and may, at regular meetings of the society, recommend changes or additions to the code. It is suggested that the committee undertake, as soon as practicable, the typifying of the Linnæan genera, as this must be the basis of all future work.

SPECIAL ARTICLES

TEMPERATURE AND VERTEBRÆ IN FISHES; A SUGGESTED TEST

IN 1862, Dr. Günther[1] noted that in the family of Labridæ (Wrasse fishes) the tropical species had 24 (10 + 14) vertebræ while those of temperate seas had a larger number, the increase being mainly in the caudal region.

In 1863, Dr. Gill showed that this generalization could be extended to other families, and that it was to "be considered in connection with the predominance in northern waters" of soft-rayed fishes" in which the increase in the number of vertebræ is a normal feature." This generalization thus included the herring, trout, salmon, smelt, cod, flounder and their relatives, and might have been extended to the sculpins, greenlings and other spiny-rayed fishes—northern types as well.

In 1864, Dr. Gill noted that the northern genus, *Sebastes*, with 12 + 19 = 31 vertebræ showed a similar relation to its tropical relative *Scorpæna*, with 10 + 14 = 24.

In various papers, the present writer has extended this generalization to numerous other families, raising it to the dignity of a "law." In general, among, spiny-rayed fishes, the tropical forms have the vertebræ 10 + 14, the northern forms, fresh-water forms, pelagic species and deep-sea representatives a larger number. In the groups of soft-rayed fishes, the vertebræ in the tropics usually range higher than 24 (35 to 43) among flounders while the subarctic species all run higher (among flounders 49 to 65). The sub-Arctic blennies have the vertebræ 75 to 100, their tropical relatives 28 to 49. Some such relation exists in every group—eel-shaped fishes excepted. These have no northern representatives and in them the whole body is peculiarly modified in accordance with their mode of life.

The facts being fairly established we look next to its explanation. Dr. Gill states (1889) that "it is simply the expression of a fact which has no cause for its being now known." He further doubts whether it can ever be ascertained.

In my own first paper on the subject[2] I suggested that the larger numbers might be primitive, and that the smaller numbers (accompanied by corresponding increase in complexity of the individual vertebræ) were the result of specialization or "ichthyization," a process which in the favoring temperature, amid intense competition of the tropics and especially about coral-reefs, brought about the more perfect or fish-like fish.

I am now, however, inclined to accept Dr. Boulenger's suggestion that the increased numbers and the lack of specialization of parts is the result of a form of degeneration, and that the lower number is a primitive trait possessed by the ancestors of most of the higher bony-fishes.

One way of testing this has occurred to me. The genus *Sebastodes* and its near allies ("rock-cod") form a large part of the temperate fish-fauna of California and Japan. These stand intermediate in characters as well as in geography between the subarctic rose-fishes (*Sebastes*, *Sebastolobus*, etc.) and the tropical scorpion-fishes (*Scorpæna*, *Helicolenus*, etc.) with their derivatives and allies.

In *Sebastes*, the vertebræ are 12 + 19 = 31; in *Sebastodes*, 12 + 15 = 27, and in *Scorpæna*, 10 + 14 = 24. The species of *Sebastes* and

[1] "Catalogue of the Fishes of the British Museum," Vol. IV.

[2] *Proc. U. S. Nat. Mus.*, XIV., 1891.

Sebastodes are viviparous, the young being developed internally and in multitudes, to be extruded when about two or three millimeters in length. The development of the young should indicate the phylogeny of the group. If the total number of vertebræ in *Sebastodes* is 24, we may infer with strong plausibility that *Scorpæna,* with its 24 vertebræ was the ancestral type. If the number is 31 we would grant this place to *Sebastes.* In either case, *Sebastodes* is intermediate.

Through the interest of Professor Edwin C. Starks, I have secured a number of young of a species of *Sebastodes* from Long Beach, California. These are very recently hatched, one to two millimeters in length. Vertebræ do not appear, but the muscular impressions which will correspond to them are 27 in number.

This agrees with the number of vertebræ in the adult of all the *Sebastodes* recorded. This test, therefore, fails to decide the question of origin, though it may be held to show that the separation of *Sebastodes* from *Sebastes* or from *Scorpæna* is really very old, and in spite of the strong resemblances of the forms concerned.

I may further note that all allies of *Scorpæna* with 24 vertebræ have 12 spines in the dorsal fin, *Sebastodes,* and its relatives with 27 vertebræ have 13, and *Sebastes,* with 30 or 31 vertebræ, has 15 or 16 dorsal spines, the numbers of fin rays corresponding in a degree to the number of vertebral segments.

David Starr Jordan

Since the above was in type, I have obtained from the diatomaceous shales of the Puente formation (Miocene) of Orange, California (E. E. Hadley coll.), a fossil fish apparently of the *Sebastodes* group. This specimen has the vertebræ about 32 in number, 10 + 20 being preserved. The head of the specimen is lost, but the fish must belong to the *Sebastinæ,* as no other forms unite the characters of stiff dorsal spines, anal rays III, 10, with small scales and the vertebræ more than 24. In other respects, this new genus (soon to be described and figured), seems nearest *Sebastosomus* Gill (*S. mystinus*). The discovery of this form is again not decisive, though it indicates the possibly primitive character of the *Sebastina* fishes having the larger numbers of vertebræ.

David Starr Jordan

THE AMERICAN ASSOCIATION OF PETROLEUM GEOLOGISTS

The fourth annual meeting of the American Association of Petroleum Geologists was held at the Adolphus Hotel, Dallas, Texas, on the 13 to the 15 of March. More than two hundred petroleum geologists and a great number of visitors were present from all portions of the United States, the association being especially honored by the presence of David White, chief geologist of the U. S. Geological Survey; I. C. White, state geologist of West Virginia; Ralph Arnold, valuation expert of the Internal Revenue Department of the U. S. Treasury, and Professor Chas. Schuchert, of Yale University.

The opening meeting of the association was called to order on Thursday morning by the president, Alexander Deussen. Gilbert H. Irish, of the Dallas Chamber of Commerce, delivered an address welcoming the geologists to Dallas. Short talks were made by Dr. David White, Dr. I. C. White; Dr. J. A. Udden, state geologist of Texas; W. F. Cummins; J. A. Taff, of San Francisco, and Leo Hager, of Houston.

The first technical session was held Thursday afternoon, attention being devoted to the geology of the oil producing districts of north central Texas. John A. Udden, chief geologist of the Sinclair Oil Company, read a paper dealing with the subsurface geology of the oil-producing districts of north central Texas, and accompanied his paper by a set of well samples and slides of the formations penetrated in some of the wells of north central Texas. Chas. R. Eckes, chief geologist of the Texas Company, gave a description of cuttings from the Duffer well of the Texas Company at Ranger, and displayed a set of samples from this well. F. B. Plummer, of the Roxana Petroleum Company, gave a description of the cuttings from the Goode well of the Roxana Company, in Young County, and the Dye well in Palo Pinto County. Wallace E. Pratt, chief geologist of the Humble Oil & Refining Company, read a paper entitled "Notes on structure of surface rocks as related to subsurface structure and petroleum accumulation in north Texas." Dr. David White read a paper by G. H. Girty, on the "Bend formation and its

correlation." Dr. Girty pointed out that the lower part of the Bend Series of the Bend shale proper belonged to the Mississippian, and the upper part of the Bend, including the Marble Falls limestone and the Smithwick shale, belonged in the Pennsylvanian, with an unconformity between the Bend shale and the Marble Falls limestone.

At 8:15 in the evening a popular meeting was held in the auditorium of Municipal Building, Dr. I. C. White presiding. A large contingent of townspeople were in attendance. The session was addressed by Dr. David White, who made a plea for the accumulation of petroleum reserves in foreign countries by the American companies so that the future of the American oil industry would be assured. Dr. J. A. Udden read a paper on oil-bearing formations in Texas, and Mr. M. L. Fuller, chief geologist of the Sun Company, delivered an illustrated lecture on China.

On Friday morning papers were read by Dr. J. W. Beede, of the bureau of economic geology of the University of Texas, on "Notes on the structures and oil showings in the Red Rocks of Coke County, Texas," by J. A. Udden, on "Observations on two deep borings on the Balcones Faults," and by M. L. Fuller, "On the water problems of the Bend series, and its effect on the future production and flooding of oil sands." T. W. Gregory, of the U. S. Fuel Administration, read a paper on "Gas conservation and distribution under the U. S. Fuel Administration."

On Friday afternoon papers were read by W. L. Matteson, giving "A review of developments in the central Texas oil fields," one by Walter R. Berger, of the Empire Gas & Fuel Company, on the "Extent and interpretation of the Hogshooter Gas Sand," a paper by Dr. Raymond B. Moore, state geologist of Kansas, on the "Correlation of the Bend." Dr. Moore's conclusions were different from those of Dr. Girty's, the collections made by Dr. Moore, for the Roxana Petroleum Company, indicating that the lower Bend, or the Bend shale proper, belongs to the Pennsylvania instead of the Mississippian. Sidney Powers read a paper on the "Geologic work of the American Expeditionary Forces." The afternoon session was concluded by a paper by Dr. Ed. Bloesch, on "Unconformities in Oklahoma."

Friday evening a banquet was tendered the association and the oil producers by the Dallas Chamber of Commerce & Manufacturer's Association in the junior ball room of the Adolphus Hotel. The meeting was addressed by Ralph Arnold, who outlined the policy of the federal government in the matter of valuation and taxation of oil properties. Mr. Arnold's address was followed by two minute talks by F. W. Shaw, David White, Chester Washburne, Judge Greer, attorney for the Magnolia Company, J. Edgar Pew, vice-president of the Sun Company, and others.

Saturday morning was devoted to a symposium on valuation methods, Dr. I. C. White presiding. Papers were read by Ralph Arnold on "Problems of oil lease valuation," by Carl H. Beall on "Factors in the valuation of oil lands," by Professor Roswell H. Johnson on "Decline curve methods," and by E. W. Shaw, of the U. S. Geological Survey, on "Valuation of gas properties."

Saturday afternoon a paper was read by Mr. E. H. Sellards, of the bureau of economic geology, University of Texas, on "Structural conditions in the oil fields of Bexar County, Texas." Dr. Schuchert gave an illustrated lecture on contacts, and Professor Roswell H. Johnson presented a "Statistical investigation of the influence of structure on oil and gas production in the Osage Nations."

The following papers were read by title:

D. F. MacDonald, "Notes on the stratigraphy of Panama and Costa Rica."

Geo. E. Burton, "Design for a log meter."

G. Sherburne Rogers, "Oil field waters."

J. W. Bostick, "The Saratoga, Texas, oil field."

Earl A. Trager, "Laboratory methods for the examination of well cuttings."

Robert T. Hill, "History of geologic exploration in the southwest."

After the reading of these papers, the business meeting of the association was held, and the following officers were elected for the coming year:

Dr. I. C. White, *President*.

Irving Perrine, of Hutchinson, Kansas, *Vice-president*.

Professor C. E. Decker, University of Oklahoma, *Secretary-Treasurer*.

Chas. H. Taylor, *Editor*.

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